his sutura vittaque submarginali, nigris; antennis nigris, articulis basalibus obscure piceis.—Long. $3\frac{2}{3}$ lin.

Hab. Brazil.

Œdionychis virginella.

Œ. oblongo-ovata, nitido-fusco-fulva, tibiis tarsisque posticis, pedibus quatuor anticis antennisque rufo-fuscis, thorace elytrisque albidis, his subrectis, lævibus, obsolete punctulatis.—Long. $3\frac{1}{4}$ lin.

Hab. San Paulo, Brazil.

XXIX.—*On Digestive Power in the* Actiniæ.

By E. W. H. Holdsworth, F.L.S. &c.

To the Editors of the Annals of Natural History.

Gentlemen,

Mr. G. H. Lewes having recently published, in 'Seaside Studies,' his views of the digestive powers of the *Actiniæ*, criticism becomes allowable; I therefore propose to offer a few remarks on the subject, and to relate my observations made during a long course of study of these animals. If I cannot always agree with Mr. Lewes, still I have no wish to depreciate the value of his researches; and my sole object in stating what I have observed is to aid in the settlement of the point in dispute.

In 'Seaside Studies,' page 207, will be found the brief query —"Do the Actiniæ digest at all?" Mr. Lewes then proceeds to give his definition of digestion, and concludes by saying,—"Thus we see that solubility and transformation are the two digestive effects, to produce which two agencies are needful—the mechanical and chemical." The first is the one adopted by the author as the *sole* means of preparing the food for assimilation in the *Actiniæ*.

In order to test the accuracy of this opinion, it will be desirable to notice what takes place when these polypes are fed. Any one who has healthy specimens in an aquarium can easily try the experiment, and will observe something like the following process:—

A piece of meat, of suitable size, after being conveyed to the mouth of the polype, is taken into the membranous tube, generally considered as the stomach, where it remains for a few *minutes* only, and sometimes barely stops there; it then passes into the general cavity of the body, and finally rests at its lowest part. The animal now fills itself with water until every part is distended to the utmost, and in this state it continues for several hours, not unfrequently for two days, the average time perhaps

being about twenty hours. Whilst in this condition, some species become sufficiently transparent to enable us to discern a great part of their internal structure; and a vertical view of the polype shows the stomach suspended like a single flat membrane from the oral disk. On looking through the sides of the animal, the food may be observed at the bottom of the great cavity—usually in the centre, sometimes a little on one side—and resting on the edges of the converging septa. In this position it stays until *whatever part of it is not required by the polype* is finally returned through the mouth. I shall presently refer to the condition of the rejected portion; but I may now inquire in which part of the animal, during the above process, does the mechanical operation take place that Mr. Lewes states is the only one by which the food is prepared for assimilation? It cannot be in the cavity of the body, for that is fully and rigidly distended with water whilst the food is within it; so that no pressure from mutual contact of the membranous septa can be exerted to extract the juices of the meat. The only time when such an agency can be employed is during the first passage of the food through the stomach, or subsequently on its return. In either case, the operation would only last for a very few minutes; and undoubtedly some pressure *may* be exercised during the process of regurgitation, as the rejected food must find its way upwards from the large gastric cavity into the free open extremity of the membranous stomach—a proceeding very much like that of getting out of a flexible lobster-pot: the contortions the polype sometimes makes whilst this is going on show the difficulty it labours under; however, it is soon over, and the morsel is ejected,—but in what condition? Is it in a state that can be accounted for by mechanical action only?

It may be here mentioned that I have various species of *Actiniæ* that have been in my possession for periods ranging from six months to as many years; and as they are generally fed once a week, and for the most part on partially cooked beef, frequent opportunities are afforded me for observing the results of feeding these animals on substantial food.

I find, then, that the remains of the meat are returned in one of three conditions, viz.:

1. Unaltered in shape, the muscular fibre intact, but with the appearance of having undergone simple maceration in sea-water, and enlarged rather than contracted in size.

2. In a rounded mass, reduced in size, not only from being rolled into a ball, but also in consequence of a partial solution of its substance; the muscular striæ faintly visible.

3. In the form of minute particles, whose aggregate bulk frequently does not equal one-fiftieth part of the original piece.

Some of these particles, when submitted to a magnifying power of 450 diameters, occasionally exhibit definite structure; but usually they are all homogeneous in their composition, and show *no trace of muscular striæ or distinct fibre.*

In the first two cases the remains are enveloped in a tenacious glairy mucus, proving them to have been ejected from the polype; in the third condition this evidence is not so decided. However, in order to avoid any possibility of mistake as to the origin of these particles, separate *Actiniæ* were placed in small glass vases, each containing only clean water and a stone bearing the single polype: the results of feeding them under these circumstances agreed with what were before observed; and when the meat was returned in the *first* condition, it was found lying on the stone or bottom of the glass; when in the *second* form, it was most frequently floating at the surface of the water; and if in the *third* and disintegrated state, it was always in that position, and immediately over the polype, unless intercepted and entangled in the mucous epidermis, as sometimes happens in the case of *Act. mesembryanthemum.*

Now let me ask, can we entirely account for these effects by the simple act of squeezing, or any other mechanical operation that can go on within the soft body of the polype? It appears to me impossible. But strong as the evidence is of the action of some solvent when only small disintegrated particles are returned, my latest experiments are still more conclusive of its existence. Of six *Actiniæ* that were fed from the same bit of meat, two threw up a few very small particles; the others, after an interval of more than a week, returned absolutely *nothing* that could be detected with the aid of a lens. These four polypes are two varieties of *Sagartia troglodytes*, a *S. nivea* and a *S. viduata*—all of them voracious species; the others are *S. venusta* and *Act. mesembryanthemum.*

No mechanical power will account for these results; and for their explanation I can only have recourse to the alternative, chemical agency, which Mr. Lewes selects as "the specific characteristic of the digestive process," but whose existence he denies in the *Actiniæ*. Of the origin and nature of the solvent I can say nothing; but I would direct attention to the glairy mucus so abundantly produced in the stomach of the polype when its surface is irritated by the presence of food. Possibly this may act only in a preparatory manner; but that it is concerned in the alimentary process there can be no question, as it is specially called forth under the excitement of feeding. This mucus appears to be a product of the stomach, since it is found coating the food before it has descended into the cavity of the body, as may be proved by giving a polype a piece of meat too large to

be entirely swallowed: on its disengagement from the animal, that portion of it which had passed the mouth will be seen thickly covered with the secretion. One frequent source of error in studying the digestive powers of the *Actiniæ* arises from the belief that all the food these animals swallow must necessarily undergo digestion, whatever the nature of that process may be; but this is far from being the case. Healthy polypes will rarely refuse anything that comes within their reach; and in most cases it is only after the object has passed through the stomach, that the animal finds out whether it is hungry, or if it has swallowed suitable food. In my own tank, a specific-gravity bubble has been gulped down by almost every specimen large enough to accomplish the feat, and by some of them more than once; but of course it is always ejected unaltered: in the same manner, proper food has been taken in and returned after a short time only, not from its being indigestible, but because the animal was not hungry. In our own case, over-eating is generally followed by indigestion; but the polype has a very ready mode of preventing such an unpleasant result. When its appetite is appeased, or its powers of digestion exhausted, it simply throws up what remains of the food; and this circumstance will explain the different states of the ejected meat, and the reason why large pieces are so often cast forth with little apparent alteration. A case has recently presented itself which shows this to be a reasonable inference. Two specimens of *Cerianthus*, which had been living side by side for a couple of years in a tall glass jar, were fed with small and equal portions of beef, and after a time each animal threw up the remains of its dinner. In one case, where the polype had been fasting for a week, the food was returned after twelve hours very little altered in shape; muscular striæ were perceptible under the microscope, and the general appearance of the meat indicated its having undergone maceration, but not compression. In the other example the refuse was cast up, after an interval of sixteen hours, in very small particles without any trace of muscular structure, and so far disintegrated as to render their removal from the water a matter of some difficulty. This polype had not been fed for six weeks; and the different results from the two animals may be fairly accounted for by the unequal intervals between their last two meals. In the true *Actiniæ* there is the same variation in the extent to which digestion is carried as in the instances just cited, and in a great measure it will bear the same explanation: we also find it partly depending on the digestible nature of the food swallowed. In the account of Mr. R. Q. Couch's experiments, quoted in 'Seaside Studies,' p. 217, great stress is laid on the fact that the delicate

skin of the ventral portions of the mackerel and whiting was uninjured, and the fine metallic lustre untouched: but I should think it is hardly necessary to remind Mr. Lewes that, in the most highly organized animals, portions of food difficult of digestion are frequently unchanged when passed off in the excretions; so that the circumstance referred to can have no bearing on the question of ordinary digestive power.

The experiments on the food contained in perforated quills also appear to me to be by no means so conclusive of the absence of chemical action as Mr. Lewes thinks; they rather favour the suggestion I have thrown out as to the nature of the mucus produced in the stomach. From the tenacious character of this substance, it would pass through the perforations in the quill far less readily than would the surrounding water, and consequently the appearance of meat freely subjected to its influence might reasonably be expected to differ from that of the partially protected contents of the quill. Such, indeed, appears to have been the case in Mr. Lewes's experiment. We are told at page 216, "On examination of the ejected quills, I found no appreciable difference between the contained meat and similar pieces of meat left in the water during the same period: in one of them, which had the meat protruding somewhat from each end of the quill, there was a maceration of the protruded ends which looked like a digestive effect;" but this effect, the author goes on to say, was due to "maceration obviously of a purely mechanical nature," because the muscular fibre was not disintegrated. Why *obviously mechanical*? After what I have observed of the partial and sometimes entire solution and disintegration of the food, may I not as reasonably ascribe "what looked like a digestive effect" to the *obvious commencement of chemical action*?

I have now mentioned some of the facts which lead me to believe in the possession of ordinary digestive power by the Actiniadæ. The cases I have noticed as bearing on the subject are but a few out of many similar ones that have occurred to me and, unquestionably, to other persons who have given their attention to the matter; for I cannot believe that when a Sea-anemone becomes an occupant of my aquarium, it is thenceforth gifted with new faculties, and learns to digest its food in a manner unknown to its brethren in other tanks or along the coast.

There are some other points relating to the *Actiniæ*, which are treated of in 'Seaside Studies,' and on which the author holds opinions at variance with those of most naturalists; but it must be observed that in almost every case he only brings forward negative evidence in support of his views; and I need hardly say that, in matters of science, such evidence is not always trustworthy.

The question of the existence of a corpusculated fluid in the *Actiniæ* need hardly be discussed. The corpuscles have been too frequently observed to leave room for any doubt of their existence. I have never failed in discovering them when I have used a power of 450 diameters; and when the fluid was taken from the body of a polype placed during three days in well-filtered sea-water, the result was only in a slight degree less decisive—the corpuscles were not quite so abundant.

At present I can say but little on the albuminous character of the chylaqueous fluid. Experiments on the subject can be satisfactorily carried on only at the sea-side, where there are plenty of healthy polypes to cut up without running the risk of destroying old favourites. On the single occasion when I boiled some quantity of the fluid, milkiness was produced; but at a later period, when small drops taken from other animals were tested with nitric acid, I could not be sure that a change of colour took place in every case. The experiments were made on animals that had been kept both in natural and filtered sea-water; but testing the character of a minute drop of fluid is an operation so delicate and novel to me, that I hesitate to give an opinion from the results I then obtained.

It must be regretted that Mr. Lewes is so positive in his conclusions from what certainly look like hasty experiments; and in questioning their soundness I am justified by the author himself when he tells us, at p. 261, "We see the necessity of a cultivated caution in the acceptance of statements in matters so complex as those of biology."

I remain, Gentlemen,
Yours very truly,
E. W. H. Holdsworth.

26 Osnaburgh Street, Sept. 1859.

XXX.—*Description of several Species of Entomostracous Crustacea from Jerusalem.* By W. Baird, M.D., F.L.S.

[With two Plates.]

In the month of July 1858, Edward Atkinson, Esq., a gentleman attached as surgeon to the consulate at Jerusalem, and who has resided in that city for some time, sent a quantity of dried mud from the pool of Gihon in Jerusalem to Mr. Denny at Leeds. By the kindness of this latter gentleman, I had a supply of this forwarded to me, which Mr. Denny states had been in all probability in a dry state for some months before it was despatched. It reached Leeds in the end of August, and the small parcel containing a supply reached me at the